

Impress of Potash -
Impress of Lead -
Arsenic -
Arsenic -
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Animal poisons -
Nitrate of Bismuth -

#8

An Experimental Essay

on the

Prussiate

Prussiate of Potash

1814

by D Burwell -

Dudley Burwell

1814

It may not be amiss to give the process by which
I prepared the precipitate of Potash. I made it in the
following manner; I first made a strong alkaline
solution, which, after being filtered was put in an
iron pot & exposed to a sufficient degree of heat
to produce ebullition; & while in that state the
Precipitate of Iron finely powdered was added as long
as the alkaline solution was capable of destroying
colour. It was then taken off the fire & filtered off
which it was put into a glass vessel, ^{& exposed} to a gentle heat
until a pellicle formed in the surface of the glass
it was then removed from the fire & set aside to
crystallize.

Doctor Love —

D. Sir,

Introduction

I called this afternoon to see you, but not finding you at home, I left my *Essay* on the Propriety of Potash. I am extremely sorry that various circumstances prevented me from prosecuting the subject to the extent you wished. So many obstacles opposed themselves, that I was forced at present to abandon the idea altogether. I have taken the liberty of keeping the original *Essay*, merely because the copy is much more neatly written, & flatter myself it will meet with your approbation. It is a faithful copy of the original, excepting a few alterations, which you suggested, together with the addition of the note I made use of in preparing the P. Potash. I remain yours &c.

April 7th 1814 — D. Sumner

mine, that they are particularly liable to err, both from the difficulties of the subjects which they treat of, and from the delusion of preconceived opinions. Obstacles inherent

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: Introduction

No inducement whatever at this time, would make me attempt an Essay on any subject, were it not rendered indispensable by the Laws & Customs of this University. From the peculiar situation of those who are subjected to the influence of the Laws relative to Graduation^{on}, it follows as a necessary consequence, that any production of theirs, must be either little or imperfect. Deprived of little or no experience of their own, they are constrained to follow the footsteps of others, which are often incorrect & very generally questionable. I do not mean to assert that no confidence ought to be placed in Medical Writers, but merely to observe, that they are particularly liable to err, both from the difficulties of the subjects which they treat of, and from the delusion of preconceived opinions. Obstacles inherent

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in the subject are only one source of the inaccuracies
to be found in Medical Authors. There is another cause
still more baneful in its influence, & to which many
of the false observations to be met with manifestly owe
their origin. I mean that predisposition to theorize which
unfortunately for the Science has insinuated itself into
most of the productions of Physicians. Far be it from
me to abolish altogether principles in Medicine - When
founded on Facts they are not only important towards
practising the art with success, but serve the addi-
tional purpose of arranging the Facts belonging to any
science in that clear & perspicuous order so necessary
to facilitate ^{its} improvement. Unfortunately however
for the Science & Mankind in general, most of the the-
ories of the present day are the effusions of Fancy, ra-
ther than the deductions of Reason. Instead of being
conclusions drawn from well authenticated facts, they
are too often formed in the imagination of the author,
facts are next sought for to support them and the

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resources of Rhetoric. If they are wanting every faculty of the mind is employed to invent plausible arguments which may serve to delude: For the appearance of Truth is sufficient to secure the approbation of most men, the majority being always more disposed to close with statements, than endure the trouble of scrutinizing their merits.

Since then the improvement of every Science in order to be permanent depends upon the development of new Facts, the necessity of Experimental Enquiry as the surest guide to truth must be apparent to the most superficial observer. It is impossible for fallacy to exist in conclusions drawn from experiments provided they are conducted with care and precision. Hence the universal aspect of mankind to doctrines founded on facts elicited by experiment. Such is my confidence in the unrivalled efficacy of this mode of investigation, in accelerating the progress of any Science, that I have ventured upon an



show of that nature. The doctly sensible of my in-
adequacy, to render it the justice which it merits, I
will not relinquish the hope, that my good intenti-
on, will at last be an apology for the imperfections
now contain'd. It was my original intention merely
to repeat the experiments of Mr Wallaston on the Trans-
mission of Light, but finding that the various results were
under the influence of my father I concluded that
it would be interesting, if perhaps useful, to com-
pare them with my own experiments. As however its effects
upon the system generally. Proffer entering directly
upon the proper subject of this essay I think it neces-
sary to explain with what view Mr Wallaston has
instituted his experiments. In order to ascertain what
the Class of Strabularia contains any Gas-
sharous matter in its composition Mr Wallaston
instituted a Series of Experiments on the Effect of po-
tass discharging daily large quantities of sugar on
their Urine not the smallest portion of the substance

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was detected. From the result of these experiments he concludes that the saccharine matter found in Diabetes Urinae exfoliates in the stomach & conveys directly from that viscous to the Residues without entering into the general circulation. To confirm the above conclusions the same Quakerman in conjunction with Mr. Marsset performed a number of experiments on the Trump. Potatoes. The point in view was, whether it could be detected when taken orally, or the blood in the same way. After experimenting several times with the article above mentioned, they concluded that it was not absorbed into the general circulation, but carried from the stomach to the kidneys, by some unknown vessels. Not having as yet been free to perform the first set of experiments made on Gracioso patients, I determined to repeat them on the Trump. Potatoes with the view of ascertaining whether Messrs. Walboston & Marsset were warranted in the conclusions contained in their paper

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of the Royal Society of London. Although my
experiments are not as conclusive as they might be
with sufficient time to push them to a greater ex-
tent, yet I am convinced from the results afforded
that this substance is taken into the general circulation
in two experiments I detected it as distinctly in the
quantity of the substance which was taken would per-
mit. It was my intention to have varied my experi-
ments on a number of inferior animals, but unfor-
tunately the press of time, & the difficulty of procuring
the proper circumstances, has prevented me from pursuing the sub-
ject to the extent which I contemplated.

As this essay will embrace subjects of a differ-
ent nature, it will be necessary to divide it into two
parts or sections the first relating to the absorption
of the Puftp Solap into the general circulation -
the second comprehending its general effects on the
system when taken into the Stomach.

Accordingly to the plan just laid down, I shall

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without further delay give us concise an account as
before of those experiments which I made oppo-
sitive to the absorption of the Puib. Potash in the
general circulation. Afterwards make some ex-
planations on the results.

Expt^l 8th At half past 9 O'clock Took 4 grs of the
Puibate of Potash in solution - in one hour after I
took 3 grs. At the end of 2 hours after taking the
first dose, I took an additional quantity of 3 grs ma-
king the whole of the substance that I had taken at
the separation of one hour amount 7 grs. Upon
examining my urine 3 hours & a half after taking
the first dose of the article I found by a strong mag-
netic portion of the solution of Iron that a slight blue tinge
was produced in it. In half an hour afterwards
being 4 hours from the commencement of the ex-
periment, I repeated it on a quantity of urine and
found by the addition of Sulphate of Iron in
solution, a very considerable precipitate of a blue

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colours to fall to the bottom of the vessel, shewing
distinctly the presence of the Sulfuric acid in the
urine. I then had myself bled, for the purpose of
ascertaining whether I could detect its presence
in the serum of the blood. On the following day,
when the serum had separated from the other parts
of the blood I removed a small portion of the solu-
tion of Iron to it & distinctly perceived a Blue colour
to ensue, proving beyond all doubt the presence of the
Sulfuric acid. Several of my friends were present,
among them Messrs Marshall & Sir Britton who
fully coincide with me in opinion.

Exp^t 2^d In the course of three hours I took in di-
vided doses 10gr of Sulph. Retash. Upon examining my
urine three hours & a half after taking the first dose
I detected the presence of the Sulfuric acid in a slight
degree, half an hour after its excretion was very man-
ifest. At this time I had myself bled, & finally
added a portion of Sulph. of Iron in solution to

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the former the same result as in the preceding ex-
periment, the not so strongly marked. I then ad-
ded a sufficient quantity of sulphuric acid to coagu-
late the serum by which the blue colour was even
a much more apparent.

Expt 3^d. - In two hours & a half I took
8 grs of the article in solution, & in four hours depo-
sition in the urine was readily detected. At this pe-
riod I submitted again to re-examination, but on trial
of the albumen test the presence of the sulphur-
ic acid was not perceptible. This circumstance I as-
cribed to the serum having been agitated & conse-
quently rendered turbid, thus being insusceptible of
the change of colour on the exhibition of the paper
test.

From the result of the above ex-
periments I hold myself warranted in withholding
my assent to the inference of Messrs Wallaston
& Warrist. I feel thoroughly satisfied that the Sulphur-
ic acid is taken up by the Globules into the general

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circulation, I convey by this route to the Kidneys,
through which organs it is eliminated from the
system. Even allowing for a moment that its pre-
sence cannot be detected in the serum of the blood,
how can we explain its existence in the urine?
The chain of inferences on which Mr Wallaston has de-
vised this Junction, rests on grounds too imaginary
& fallacious to warrant a conviction of their validity.
It is certain that they have never yet been dis-
covered either by injection or the aid of microscopic in-
vestigation though employed by Anatomists renowned
for the keenest discernment & unremitting industry.
This circumstance alone is sufficient to prove that
their alledged existence is wholly unfounded as
its presence in the urine is susceptible of the most
conclusive demonstration, the inability, to detect
it in the blood of such is the fact, may with more
reason be ascribed to the imperfect state of phy-
sical knowledge than to its actual absence.

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We have ever been able to discover the specific differences between Marsh-miasmata & common atmospheric air, & yet mantles are universally, aware that their properties are essentially distinct. The knowledge on the subject is derived from observation directed to their effects on the System. By which it is incontrovertibly proven that the one is endowed with some peculiar property to which its noxious effects are referable - Although ignorant of the nature of this agency, we are nevertheless fully apprised of its existence. It is very well known that the Marsh of Mercury may be given in solution so much divided as to produce the serious impregnation of the system without our being able by any known chymical test to ascertain its presence. This circumstance is sufficiently strong to point out the necessity of deliberate caution in drawing conclusions from such premises. From the above mentioned experiments I am thoroughly con-

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proved that the assumption of vessels going direct-
ly from the stomach to the Kidneys, is wholly gra-
tuitous & without a fact to support it.

(With respect to the seat of the disease called
Diabetes, I coincide with Mr. Wallaston in sup-
posing that it is purely a disease of the stomach. I
am led to this opinion 1st From its often attend-
ing other diseases of that viscus such as Hyper-
taemia, Hypochondriasis & Dyspepsia. 2^d From the
venous appetite, indigestion & colic which
almost invariably attend this complaint. 3^d
From its general occurrence in those whose con-
stitutions have been shattered by intemperance in
drinking and other excesses, the effects of which are
primarily seated on the stomach. In addition
to the arguments derived from the disease itself, I
have the observations of several Practitioners of the
highest eminence, which prove beyond all doubt
that the blood of Diabetic Patients contains su-

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ger in its composition. From the experiments of
(J) Tolson we learn that he detected sugar in the
Urine of Thomas Laburn, under Diabetes. The au-
thority of this gentleman alone would be suffi-
cient to give unquestionable validity to the factum
if not supported by the observations of others of
equal respectability & weight. I allude to the
observations of Dr Rolfe & Mr Crickshank these
gentlemen are fully persuaded that they ascer-
tained the presence of sugar in Diabetes Gluc by test-
ing the serum, which evidently imparts a faint
tint. I am extremely happy that I am in posses-
sion of a fact from a source entitled to the highest
credit, which goes directly to confirm the above
opinion. The fact was communicated to me by
Professor Denton, whose talent for close & correct
observation is already known & justly appreciated
by the scientific world. A Patient of his labour-
ing under Diabetes was found to discharge daily

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large quantities of feccharine matter in his Urine.
On having been told of fasting the Urine after the
the presence of Sugar was distinctly exhibited. The ex-
periment was repeated several times & with the
same result. - From the facts related above I think
myself justified in the conclusion that the disease
is primarily seated in the Stomach soon affecting
the whole of the Chylopoietic viscera, & that it de-
pends upon an imperfect assimilation of the ali-
ment taken into that viscus. - The food is hereby un-
der unfit for the purposes of Nutrition, & when ta-
ken up by the Lactals & conveyed to the general
Circulation it is rejected by the nutrient powers of
the system, & eliminated through the kidneys. The in-
creased action of these organs is generated in the efforts
of nature to free the vascular system of that effluvia of
Blood which is constantly pouring into it thro' the me-
dium of the Lactals. - I shall close this part of
my subject by observing that the disease appears

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ance of the Kidneys is entirely owing to a Law of the
universal Economy termed Sympathy. By it this
action is increased according to the exigencies of the
system, & being kept up for any length of time, must ne-
cessarily produce more or less disarrangement by the continuous
stimulus which it imparts. They have no agency
I believe in the formation of the sugar incident to Dia-
betis urinae, but merely serve to remove it already
formed by the Pancreas, & to throw it out of the System.

Part II^d.

I come now to the second part of my subject, or
the influence which the Purg. Potass when taken intern-
ally exerted on the Puls. & system in general. I shall re-
late the experiments in the order they were performed,
making some few observations on each & afterwards
make remarks as may naturally grow out of the
general result.

Exp^t 1st. Took one & a half grs of the Purg. Potass.

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in solution my pulse beating 79 strokes per minute.

In 11 minutes after it beat 64 strokes - in

12. 25. 25. 30. 35. 40. 45. 50. 55. 60. 65. 70. It will be
taken 64. 62. 60. 60. 58. 62. 62. 64. 65. 65. 70. 72.

hence that my pulse in the above experiment was re-
duced in frequency 11 strokes, that is, decrea-
sed between 35 & 46 for the space of 14 minutes, ^{than} gradually
rose to 79 which was the standard at the commencement
of the experiment. -

Exp^t 2^o. I took a grain of Quinine in solution, pulse beat
89 strokes in a minute, in ^{12 min.} 12. 20. 25. 30. 35. 40. 45. 50. 55. 60. 65. 70.
72. 75. 80. 80. 75. 70. 74. 74. 74. 76. 74. 72. 75. 76.

By a reference to the last experiment it will be found
that my pulse during the first 20 minutes was re-
duced only two strokes; it then suddenly fell from 80 to
75 being seven strokes less than at the beginning of
the experiment. It remained diminished in frequency,
between 74 & 80 for about 35 to 40 minutes, I suppose.
Why the operation of the article was not as evident in this
experiment during the first 20 minutes as in the pre-
ceding may appear strange. In the first place my

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It was preternaturally frequent being 87 strokes
per minute at the beginning of the experiment. I
suspect that it was thus increased in frequency by
something which I had either eaten or drunk previ-
ously to the exhibition of the dose, or perhaps there was a
slight degree of fever induced by the usual causes of
such disorders; it being the season of the year when
the system is always more or less impregnated with
the seeds of Fever. If what I have said be the true
cause of the preternatural state of my pulse, & I am
disposed to think the position well founded, it may be
readily explained why the medicine did not show its
effects more promptly. After being taken into the sys-
tem it had to combat the excessive excitement of
the system, which would retard the development of its
power more or less according to the degree of fever. This
appears to be the true solution of the problem, from the
further circumstance of its producing an evident
reduction of the pulse in the course of 25 or 30 minutes

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continuing to do so for a considerable time afterwards.

Exp^t 3^d. I took 2^{gr} in solution my pulse
beating 76 strokes ¹/₂ minute. In 10 minutes it beat
In ¹/₂ min. 15. 20. 25 30. 35 40. 45 50. 55 60. 65 70
Strokes 74. 64. 64. 64. 64. 64. 62. 62. 69. 72. 72. 72.

It will be seen in this experiment that my pulse was
raised in frequency at one time 12 strokes, that it
continued vibrating between 64 & 66 for 20 minutes,
& then very gradually rose. During the vibration of
the article my pulse was very irregular in point of
time between each vibration, accompanied with con-
siderable depression & slight pain about my breast.

Exp^t 11th. I took 5^{gr} of the article in solution, my
pulse beating 72 strokes ¹/₂ minute. In 10 minutes
after it beat 69. In ¹/₂ min. 15. 20. 25 30. 35 40. 45 50. 55 60. 65
Strokes 69. 66. 69. 69. 66. 66. 64. 66. 64
70. 75. 80 85 90 105. 115. 120.
60. 60 59. 54 54 54 51. 54.

In this experiment it will be
observed that my pulse was not so suddenly reduced
in frequency as in the preceding, but that its ultimate
impulsion was much more powerful, both in diminu-
ishing the frequency of the pulse, & in the time of its

[illegible]

duration. For the first 40 minutes my pulse
was only reduced in frequency 3 strokes, i. e. 20
minutes more it gradually fell to 64 being 10 strokes
less than at the beginning of the experiment. At the
expiration of 15 minutes more it will be found to
have lessened 3 strokes. It now fell to 55 strokes p.
minute. Remained stationary for 35 minutes; except-
ing once during the process when it fell to 51 which
was nearly two hours after the article was ad-
ministered. I examined my pulse three hours af-
terwards & found it still lessened in frequency.
During this last experiment I frequently felt an un-
easiness in different parts of my head, my face
was alternately flushed & very pale, considera-
ble weakness of my eyes, & disposition to keep my
mind somewhat dejected, with general debility &
indisposition to move about. Towards the close of
the experiment a considerable Diaphoresis appear-
ed, & likewise twitching of the Carotids. My

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hals during the operation was away much dimin-
ished both in volume & force, I was marked by
considerable, uneasiness in relation to the time of
its vibrations, with oppression at my breast. My
urine was increased in quantity, but a good deal
difficultly attended its evacuation. About
three or four hours after the article was taken
my bowels became affected & discharged liquid
stools with ~~which~~ with some degree of griping.

In reviewing the results of the preceding ex-
periments the first circumstance that attracts
our attention is the uniform effect of the arti-
cle in reducing the frequency of the pulse without
in any instance, raising it above the natural
standard. It moreover exerted considerable influ-
ence on the volume & force, producing general de-
bility of the system, & rendering it disagreeable and
unpleasant to use any exertion either of body or
mind. A difficulty of breathing with tightness

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A slight pain, was particularly exemplified in the last experiment. There was during the same experiment considerable uneasiness of my heart, at times amounting to pain with weakness of my eyes, disposition to sleep. Towards the close a Diarrhoea was very distinct, appearing first in the bowels, breast, palms of the hands, & then gradually spreading itself over the surface of the body. At this time all the disagreeable feelings above mentioned disappeared. Several hours after taking the last dose of the article when all the above symptoms had subsided I felt some degree of pain & griping in my bowels accompanied with a discharge of a liquid nature, which afforded relief & restored them to a state of quiescence. About the same time my urine was increased beyond the natural quantity, but great difficulty was experienced in getting the bladder having in some measure lost its energy. This circumstance was rapidly

[illegible]

stomach, the inclination frequently occurs but the bladder was unable for several minutes to perform its function. This inability frequently continued for one & twenty hours. At no time during the whole series of my experiments was the slightest nausea, or any alteration in the state of the stomach manifested. Although the generally received opinion of the present day respecting the primary operation of all medicines on the human system is, that they increase action, still I am compelled by the results arising from the preceding experiments to dissent from the application of this maxim to every article of a medicinal nature. I feel fully authorized in asserting that the sensible operation of the Drugs Potass on the system is that of a sedative strictly so called. It uniformly exhibits the frequency of the pulse without in any instance increasing it beyond the natural force.

Its general effects on the system never extend to

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from the same point beyond all manner of
doubt. In order to remove every doubt, opposite
to the utmost accuracy in the preceding obser-
vations, I procured the favour of my friend and
now Student Mr Wm S. Marshall of S. Caro-
lina to examine the state of my pulse at differ-
ent periods during the experiments, who I am
well assured made use of all the care & precision
which the nicety of the subject demanded.

It is ascertained by analysis in the hands of
the most respectable Chemists that Kunkin uric is a
constituent of several of the Narcotic Articles of
the Materia Medica. Finding it always present
in these articles which have been examined by,
them, they have analogically concluded that it is a
necessary part of the composition of all Narcotics.
From the effects exerted by the Pure Potash on
the system, & the effects of Opium & other Narcotics
so closely, I am induced to credit the suppo-

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tion that the peculiar virtues of such articles are owing to the proper acids they contain. If this opinion be well founded the Stup. Plap deserves to hold a higher rank than it has hitherto been given in the field of Practice. Not having used it whole myself, nor seen it used by others, I am totally unprepared to say any thing in relation to its efficacy. Judging however from its general effects I think it would be well adapted to those cases of increased excitement when depletion, having been pushed to a certain extent, is contraindicated by the debility which its further prosecution would endanger.

In its operation it resembles Opium so very closely that I am inclined to think it might be advantageously exhibited in many diseases where that noble medicine is found efficacious. - Perhaps by administering this remedy we may procure the beneficial effects

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Opium without its disadvantages, such as
stupor, languor & which almost always follow
its exhibition. Though the article indeed produced
general debility & a disposition to sleep during its
operation, yet in a few hours all these disagreeable
effects passed off, leaving me in every respect as
well as before I had taken it.

From its acting so promptly in reducing ex-
citement, I think it might operate as an effica-
cious remedy in Hemorrhages of the active
kind. This however is entirely speculation hav-
ing no facts to support it; nor is it likely
that a trial will ever be made with it while
other remedies exist of far less doubtful efficacy.
Notwithstanding the virtues of this substance
in relation to the cure of diseases has not yet been
ascertained, still from a survey of its general
operation on the system, I think it by no means
improbable that it will be found a highly

I have been thinking of writing to you for some time but have been so busy that I could not find time. I am now in the city and am very busy with my work. I am well and hope this letter finds you the same. I am very much interested in the progress of the cause and hope to see you soon. I am, dear friend, very truly yours, Wm. Lloyd Garrison.

valuable acquisition to the materia Medica, and
at no distant date be clasp'd with these
happy means we already enjoy of alleviating
the maladies incident to our nature. —

Before Gentlemen I close this very
imperfect essay, I beg leave to tender my sin-
cere acknowledgements for the information I have
received from your lectures respectively, and
may you long live to enjoy that fame & repu-
tation which you have so justly acquired. —